

Remarks on the treatment of inflammation, with special reference to pneumonia.

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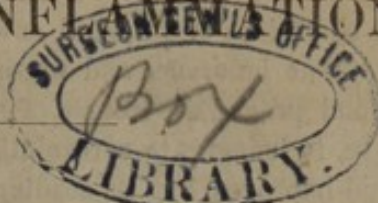
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Lawson (L. M.)

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TREATMENT OF INFLAMMATION.



THE history of medicine affords ample evidence that the opinions of physicians, in past ages, were subject to numerous and extensive changes, constituting complete revolutions. The want of a scientific basis enabled the bold or ingenious innovator to supplant the received opinions of the day, and to substitute a new for an old error. But these revolutions are of ancient date. Modern medicine admits of improvement, but not revolutionary changes. In the present, or scientific age, we distinguish between isolated facts, and a connected series of events. The former are accidental and often deceptive; the latter, duly classified and arranged, embrace the entire series of facts and events bearing on the subject, and therefore constitute a science. The shepherd of Melampus could prescribe steel and wine for impotence, or hellebore for phrenzy, without any scientific knowledge of the nature of the disease, or the action of the remedies. And even Hippocrates, by his remarkable powers of observation, was often capable of forming an accurate prognosis, while he possessed but little knowledge of anatomical structures or pathological changes.

In the revolutionary ages, ill-observed facts and crude generalizations too often served as the bases of imposing systems, which reigned supreme until supplanted by others, more specious, but equally false. Thus, the Empirics subverted the doctrines of the Dogmatists, while the Methodists intervened and overturned the theories of their predecessors. And even in comparatively modern times we find the Mathematical school subverting the Chemical, and the Vitalists superseding the Humoralists. In these speculative systems, a single misinterpreted fact may occupy the place of a science; while obscure technicalities and bold asseverations, are mistaken for rational explanations. The elixir vitæ of Paracelsus, and the antidote of Mithridates, were as probable as the aurum potabile of Bacon, or the similia similibus curantur of Hahnemann; and the Androides of Albertus Magnus was as nearly a scientific revelation as the archæus of Van Helmont or the anima of Stahl. While the cell-evolution of Bennett, although intrinsically true, may, by ingenious misinterpretation, like the debility of Brown, and the spasm of Cullen, become the basis of an erroneous system.

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But, within the scientific period—that is, since the successful cultivation of general and pathological anatomy, organic chemistry and general pathology—complete revolutions no longer take place. The attempts to introduce new systems usually result from some sinister cause. The efforts of Brown to subvert the doctrines of Cullen; the visionary and absurd hypothesis of Hahnemann; the crude system of the illiterate Thomson, with its more elaborate but equally empirical fructifications, must be regarded, severally, as the ebullitions of personal envy and jealousy, of dishonest pretensions, and extreme ignorance and presumption. They failed to subvert the regular and progressive course of scientific medicine; nor is it possible, in this age, to inaugurate a revolution. The science may and will be modified and improved, but its leading principles do not admit of sudden and violent subversion.

In opposition to this opinion, however, a recent attempt has been made to destroy some of the leading principles of therapeutics, and which, if carried into effect, would constitute a revolution. I allude to the doctrines recently propagated by Professor John Hughes Bennett, of Edinburgh, seconded, to a certain extent, by Professor Todd, of London, and partly in accordance with the views and practice of the sceptical school of Vienna. The essential features of this doctrine are, that inflammation cannot be properly treated by depletion, the increased flow of blood being a sanative process, but that the powers of the system should be sustained; hence, instead of bloodletting, we should resort to wine and beef-tea! In accordance with this opinion, we find Dr. Bennett treating *pneumonia* with an insignificant amount of salines, together with wine and beef-tea; while Dr. Todd administers brandy in the very commencement of *pericarditis*. Dr. Bennett, however, must be regarded as the propagandist of this doctrine; for while others rely on vague and deceptive statistics, he attempts to reduce the system to definite principles, and thus to place the whole on a scientific basis.

In a work entitled, *Clinical Lectures on the Principles and Practice of Medicine*, issued simultaneously in Edinburgh and New York, Dr. Bennett declares that the diminished employment of bloodletting and other antiphlogistic remedies of late years, does not arise from a change in the *type* of disease, as supposed by Drs. Alison, Christison, Watson, and the profession generally, but that the present advanced state of pathology and diagnosis has proved the former treatment to have been erroneous. Hence he concludes that inflammation is the same now it was in former years; and, consequently, that the experience of our predecessors, such as Cullen and Gregory, was based on false views of pathology and errors in diagnosis, and should not be received as guides. And to make the attempted revolution more complete, Dr. Bennett, in a stealthy paragraph, announces that mercury does not promote the absorption of lymph, and is, therefore, in that sense, useless in the inflammatory exudations. Without stopping

to argue these questions, I shall introduce his third proposition, which will fairly open up the subject. It is thus stated:—

“That the principles on which bloodletting and antiphlogistic remedies have hitherto been practised are opposed to a sound pathology.”

In enumerating the principles on which bloodletting has *hitherto* been practised, Dr. Bennett assumes that it was employed for the following purposes: 1. To diminish the *materies morbi* of the blood; 2. To lessen the quantity of blood which flows *to* the part; 3. To diminish the increased quantity of blood *in* the part; 4. That the character of the pulse was assumed to be an index to the quantity of blood which ought to be drawn. These several points Dr. Bennett proceeds to controvert with great earnestness, and finally reaches the conclusion that practitioners have hitherto been employing their energies in a wrong direction, and that inflammation cannot be relieved by depletion. Let us examine his arguments in the order in which they occur:—

1. *“Can the materies morbi in the blood be diminished by bleeding?”*

This question Dr. Bennett answers emphatically in the negative; and who, let us ask, will assume the affirmative? Certainly no enlightened *modern* practitioner would seriously advocate an opinion which is a mere figment of ancient humoralism, now utterly repudiated by the world of pathologists. Still, Dr. Bennett declares that the “idea of diminishing the morbid matters in the blood has not only descended from Hippocrates to the days of Sydenham, but has come down from his to our own times.” It is certainly untrue that physicians of the present day employ bloodletting for the purpose of removing the *materies morbi* from the blood; and it would appear, indeed, that Dr. Bennett has thus exhumed an obsolete idea for the purpose of giving force to his own untenable positions. So far, indeed, is this charge from the truth that we need not stop to argue its correctness, but merely to brand it as a libel on the intelligence of our profession, and a disingenuous effort to sustain a favourite theory.

2. *“Is it good practice to diminish the flow of blood to the part?”*

This question, asked by Dr. Bennett, involves his main views in regard to the nature of inflammation, and his deductions as to the proper method of treatment. Of course he decides that it is *not* good practice to diminish the flow of blood to an inflamed part, but that, on the contrary, it is in accordance with sound pathology to augment the determination to the diseased structure, for the purpose of aiding certain ulterior changes connected with cell-growth. The main proposition will be regarded by most practitioners as a novelty in therapeutics, and it will be curious to ascertain how the author can produce even a shadow of reason to sustain an opinion so completely at variance with the received doctrines of the day.

In the first place, Dr. Bennett assumes that the *throbbing* witnessed during inflammation, is a *result* and not a *cause* of the morbid action, and that the blood inducing this phenomenon is not *forced* in by a *vis a tergo*,

but is *drawn* in by a *vis a fronte*. And it is further assumed, that this superabundance of blood which is drawn into the part during inflammation, is a special provision of nature to promote cell-growth, whereby the exudation is broken up and rendered capable of being removed, either externally (by suppuration), by passage into the blood (absorption), or by excretion through the emunctories. To accomplish these changes, it is contended that an increased quantity of nourishing fluid is demanded, for example, such as occurs in the ripening of the Graafian vesicles; during the growth of the stag's antlers; in the mammæ during the secretion of milk; in the process of dentition; and during the ascent of sap in plants. Hence, Dr. Bennett declares that as this cell-growth occurs during inflammation, "it is absolutely imperative that the part in which these nutritive changes go on should receive more blood, to enable it to accomplish them." Bleeding, therefore, is an improper agent in inflammation, as it interferes with the nutritive acts necessary to accomplish a cure.

This is a condensed statement of the views entertained by Dr. Bennett, in regard to the nature of inflammation, and the process of cure. It will not be hazarding too much, I think, to assert that the whole statement is at variance with the phenomena of inflammation, no less than the soundest principles of practical experience and observation.

It is asserted by Dr. Bennett that the *throbbing* witnessed during the process of inflammation is produced by the blood being *drawn into* the diseased part, and that this increased flow is a provision of nature to favour cell-growth, and should therefore be encouraged. But unfortunately for this theory, it is all pure assumption, which is not only unsustained by any direct facts, but is actually disproven by certain considerations which I shall proceed to state.

A little attention to what occurs during inflammation will place this question in a proper light, and relieve it of all doubt or misapprehension. The inflammatory process is complex in its character, consisting in part of deranged circulation, which results in a complete *stasis* in the capillary vessels; and, as a consequence of this state of obstruction, the blood is unable to pass through the part, although an increased quantity may be forced into the structures. The result, therefore, is, that the blood being unable to pass, the arteries adjacent to inflamed tissues become dilated, and *throbbing* ensues. So long as the vessels remain unobstructed, and the blood flows freely through them, the phenomenon of throbbing cannot occur; but the moment the impediment is established, the *vis a tergo* causes the symptom in question. The power, therefore, which induces the throbbing is not a *vis a fronte*, by which the fluid is *drawn into* the part, but is in fact a *vis a tergo*, propelling the blood against an obstruction, the lateral pressure producing dilatation and pulsatory movement called *throbbing*. In making these observations I do not intend to assert that the phrase, "*ubi irritatio, ibi affluxus*," is untrue; on the contrary, it is evidently a correct

observation that an irritation invites an afflux of fluids to the part. But this afflux is a mere initial stage, and induces only swelling and redness, until *obstruction* supervenes, when throbbing becomes a prominent symptom. It is not true, therefore, that the throbbing is evidence of a sanitary process, whereby nature supplies a large flow of blood in order to remove obstructions; but it is a *result* of an impediment to the free passage through the capillaries, and consequently the distension of the vessels adds to the morbid condition by introducing more blood than can be transmitted or consumed in the part.

This explanation, so obviously correct, is entirely at variance with Dr. Bennett's hypothesis. He assumes, it will be remarked, that the throbbing is an evidence of determination to the part, the fluid being *drawn into* the inflamed tissues for the purpose of aiding cell-growth in the transformation of the lymph exudations, and is therefore a sanative process. But instead of blood being *drawn into* the tissues for this or any other purpose, there is in truth complete *stasis* in the part, and consequently no blood can pass through the vessels involved in the morbid action.

Dr. Bennett's view of inflammation is that it begins *outside* the vessels, and that an *active* force draws the blood *into* the capillaries, and the lymph *through* their walls. All this might be admitted, and yet it would neither sustain his view of the cause of throbbing, nor would it prove the impropriety of bleeding. The first tangible act of the inflammatory process is a determination of blood; and in relation to the present question it is quite immaterial whether the irritation originates within or without the vessels. The ultimate effect, however, is to induce stasis, as before explained, and this gives rise to the *throbbing*; and hence, the latter phenomenon is no evidence of an effort of nature to remove an obstruction by cell-growth, but is simply the sign of an obstruction preventing the free passage of blood.

In attempting to sustain the idea that an increased quantity of blood is a provision of nature to cure the inflammation, Dr. Bennett introduces, as analogous conditions, the increased flow witnessed in the ripening of the Graafian vesicles, in the secretion of milk, etc. These illustrations are peculiarly unfortunate, for they are simply physiological acts, and therefore cannot be regarded as analogous to the determination of blood connected with a morbid process. In fact these physiological acts do not bear the remotest resemblance to the changes which occur in the capillary vessels during inflammation. In the physiological state, there is simply an increased flow of blood; while in the pathological condition, the vessels become *obstructed*, and morbid *exudation* finally takes place.

If the determination to a part is restrained within physiological limits, then the active circulation, as in the secretion of milk, is beneficial; when the excitement is *morbid* as in inflammation, the result is a morbid abundance of blood. But it may be assumed that bloodletting, even when it is not influenced by medical

circulation, and therefore should be *reduced*. There is too much *excitement*, and too abundant a flow of blood, both of which are abnormal conditions, and should be diminished. But the question arises, can bloodletting diminish this morbid condition?

In the third place Dr. Bennett asks:—

3. "*Can bloodletting diminish the quantity of blood in the inflamed part?*"

This question he answers in the negative. Most assuredly, then, his previous argument, that bloodletting proved injurious by lessening the quantity of blood, and thereby checking cell-action, is wholly gratuitous; but not to take advantage of this evident discrepancy, we will proceed to show the fallacy of this third argument against bleeding.

Dr. Bennett expresses the opinion that the idea of lessening the amount of blood in the inflamed tissues constitutes one of the principal objects in bloodletting, and he denies that such an effect will follow. The chief reasons which he assigns for this opinion are: first, that the blood is stagnated in the inflamed part, and therefore cannot be withdrawn or lessened; second, that if it could diminish the blood in the vessels, it would not assist in removing the exudation which is outside. If the bleeding is large, he argues that it will prostrate the vital powers, and thereby prevent the transformation of the exudations; if the quantity abstracted is small, it cannot produce any effect. He thinks, also, that local bleeding is inefficient, because it cannot be explained on the ground of any direct anastomosis between the vessels of the surface and those of the inflamed part! "From these considerations," he says, "*it follows that neither general nor local bloodletting can possibly be supposed to diminish the amount of blood in internal parts affected with inflammation.*"

It would be difficult to crowd a greater amount of error in any similar number of words. Dr. Bennett appears to have misconceived the whole spirit of depletory practice, and constantly combats monstrous errors, which no sound pathologist for a moment entertains. In the first place, the chief object in abstraction of blood for the relief of inflammation, is not, as Dr. Bennett asserts, designed to withdraw the blood-corpuscles which have already become adherent, nor to remove the lymph which has exuded outside the vessels; but it is designed *to retard and limit, or even arrest, both the processes of stagnation and exudation*. And this is accomplished by diminishing the quantity of blood sent to the part, by lowering excitement, and thereby checking exudation. The stasis of blood, and the exudation of lymph, are evidently proportioned to the force of the circulation and the degree of inflammatory excitement; indeed, if there is any other law than the degree of excitement directly favouring these actions, I am at a loss to conceive its character. If the excitement and determination of blood are slight, the result will be merely active hyperæmia, or at most a *mod. ibi affluxus*," is untrue; on the contrary, it is evidently a correct "

of blood, and a very small amount of exudation ; but if the grade of action is high, a large amount of blood will be forced into the diseased part, stasis soon becomes extensive and complete, and the exudation of lymph active and abundant. And who can doubt that the destructive tendencies of inflammation, and the difficulties in securing resolution in ordinary cases, are precisely proportioned to the primary conditions of high action and copious exudation ? For example, a moderate grade of pneumonia, limited in extent and intensity, is far less likely to prove fatal, than where a large amount of blood, in the form of active inflammation, is precipitated upon the pulmonary structures, giving rise to a copious effusion of lymph, blocking up the air-cells to an extent incompatible with life.

Now, it is precisely in these conditions of active inflammation that we resort, in the early stage of the disease, to bloodletting and other antiphlogistics, but not for the purpose, as Dr. Bennett states, of removing stasis and lymph, but to prevent or limit these conditions. Take, again, as an illustration, pneumonia, to which Dr. Bennett so often alludes. In the beginning of the disease we find the patient labouring under inflammatory fever, with a full and strong pulse, hurried breathing, cough, etc. ; and a physical exploration of the chest reveals the crepitant rhonchus, with the accompanying signs of pulmonary engorgement and effusion of lymph, but short of consolidation. Under these circumstances, it is abundantly established by clinical experience (even by Dr. Bennett himself, as we shall see hereafter), that bloodletting, general or local, together with other antiphlogistics, will moderate the fever, reduce the force of cardiac and arterial action, *and thereby diminish the flow of blood into the inflamed tissues, and proportionally lessen the exudation of lymph ;* and hence, if consolidation ensues, it will be comparatively limited in extent, and therefore the more readily resolved.

But Dr. Bennett's arguments apply to an altogether different stage of disease, and an altered degree of excitement. The cell-transformation, for which Dr. Bennett desires so much blood, and on account of which he objects to depletion, is nothing but *resolution*, and does not commence until the force of disease has subsided, and consequently the active stage of inflammation has ceased ; hence the period when the vital forces are required to assist the changes connected with the absorption of the exudations, the organization of tissues or the formation of pus, is not the stage in which any enlightened practitioner, resorts to depletion. On the contrary, we deplete in the stage of *excitement*, and support the strength during the stage of resolution or cell-transformation. Dr. Bennett, therefore, misrepresents the practice on this subject. No judicious physician would think of depletion in the stage of resolution ; and no sound pathologist would attempt to increase or sustain the excitement during the forming or active stages of inflammation.

But it may be assumed that bloodletting, even *in the stage of excitement*, will so *influence the disease*, as to be *uninfluenced by medical treatment*.

far prostrate the system as to prevent the recuperative powers from removing the exudations. To this view I would oppose the argument, that the force of disease, unchecked by antiphlogistics, will tend to more complete and hopeless debility, than arises from judicious depletion; and the difference in the two conditions is, that the prostration from disease alone is of difficult removal, while the debility arising from depletion is more likely to be temporary, and consequently restoration can be the more readily secured by stimulants and nutrients.

The resolution of pneumonia, or the cell-development of Dr. Bennett, is, evidently, a process of *degeneration*; that is, the breaking up of a consolidated lung is a retrograde action, by which the cells liquefy and absorption takes place. Here, as in other examples of inflammation, the two forms of exudation, coagulable and corpuscular lymph exist, and organization or liquefaction, as in other instances, depends on the predominance of one or the other. Mr. Paget observes that the larger the proportion of *fibrin*, the greater the probability of adhesions and indurations; and the larger the proportion of corpuscles, the greater the chances of suppuration or other forms of degeneration. Now, it will hardly be denied, that bloodletting, by lowering vitality, will lessen the amount of fibrinous exudation, and relatively increase the corpuscular, and thus diminish the chances of permanent consolidation. Bleeding, therefore, favours resolution, and hinders adhesions, consolidations, and indurations.

I am not unmindful of the assumed therapeutical law, that bleeding does not immediately reduce the proportion of fibrin. This may be true as an independent proposition; but it is equally obvious, that a lowered vitality diminishes the plasticity of fibrin, and gives the predominance to corpuscular exudation. Hence, bleeding does, in that sense, diminish fibrinous exudation, although it may not be immediately perceived in its proportion in the blood. Bleeding, therefore, becomes appropriate in pneumonia, for the act of resolution is a process of degeneration, and requires a lowered action; indeed, resolution does not commence until the activity of inflammation has ceased.

It may safely be affirmed, therefore, that the determination of blood to a part, during the early stages of inflammation, is not a conservative act, designed to favour cell-growth; but it is in fact a morbid process, the essential disease itself, ultimately causing the very products which require cell-action for their removal. And it is equally evident that depletion may lessen the quantity, and alter the quality of blood flowing into the inflamed tissues, and thereby limit the exudation, and the tendency to permanent consolidation. Hence the objections to bloodletting apply to the stage of resolution, and not to the early and active periods of the disease. Dr. Bennett's arguments, therefore, are fallacious, and his conclusions unsound.

Dr. Bennett's fourth proposition reads thus :—

4. "*That an inflammation once established cannot be cut short, and that the only end of judicious practice is to conduct it to a favourable termination.*"

This proposition constitutes a fundamental element in the non-bleeding platform; but its force is greatly lessened by the fact that even if true, still the violence of the inflammatory process might be greatly mitigated, and a favourable issue secured, although the duration of the morbid action might not be materially abridged. But, passing by this view, let us inquire how far the proposition is intrinsically true, that inflammation must necessarily run its full and complete course.

Dr. Bennett proceeds to show that there are certain forms of disease which cannot be cut short by treatment, but are permitted to run a natural course, among which he mentions typhus fever and smallpox, and adds: "It appears to me that the same rule ought to hold with regard to the internal inflammations," etc. It must certainly be regarded as a very unsound doctrine that simple inflammation is subject to the same pathological laws that govern *specific* disease, and that because the latter run a natural course, no effort should be made to curtail the duration of the former. In support of the opinion that inflammation does not require nor admit of depletory treatment, Dr. Bennett introduces, as analogical conditions, the changes connected with fractured bones, divided tendons, and the resolution which occurs in contusions and lacerations. But how widely different are these processes of repair, which hardly vary from physiological growth, from the morbid action occurring, for example, in pneumonia! In the latter disease, the tissues are filled with blood, copious exudations obstruct the air-cells, and a vital function thus becomes rapidly impaired. Surely there is no analogy, in a true pathological sense, between such a condition, and that which occurs in the repair of a fractured bone, or the union of a tendon after subcutaneous section. It certainly does not follow that, because the surgeon would not bleed in ordinary cases of repair, the physician should equally abstain from depletion in cases of severe visceral inflammation. If, however, we admit the analogy, the difficulty will not be removed; for, as shown by Mr. Paget, *repair* does not commence while there exists a high grade of inflammatory action, and it is only after a subsidence of the morbid state that the physiological process of repair begins. And so, too, of visceral inflammation; resolution does not begin until inflammation ceases or abates, and hence the necessity of appropriate depletion.

But one of the most palpable errors into which Dr. Bennett has fallen is the supposed similitude between specific disease and simple inflammation. What modern pathologist could for a moment believe that essential fever and local inflammation are governed by the same laws, or that smallpox and pneumonia must equally run a natural course, uninfluenced by medical

treatment. It has assuredly been reserved for the great Edinburgh pathologist to utter this profound doctrine, and as an evidence, too, of the present advanced state of pathology. The doctrines of Broussais, of Clutterbuck, and of Pinel, would be no more absurd at the present day, than this idea of a parallelism existing between essential fever and local inflammation. The former is the result of specific causes, which requires a definite and even fixed period for its evolution; the latter may result from general or non-specific causes, and therefore requires no definite or fixed period for its development and decline. Thus, the duration of typhoid fever uniformly presents an average of three weeks, being incapable of reduction below that period; while pneumonia varies in duration from four or five days to quadruple that period. Certainly the similitude is not very striking between specific and non-specific disease, and any opinion based on such an assumption is wholly fallacious.

Still Dr. Bennett insists, as a fundamental proposition, that inflammation once established cannot be cut short; and hence bloodletting—which would not only fail to limit the inflammatory action, but would prove injurious by interfering with the transformations of the exudation—should be abandoned. Dr. Bennett's opinion that inflammation cannot be cut short is evidently based on his peculiar definition of that process, which is, that inflammation consists in "*an exudation of the normal liquor sanguinis.*" Therefore, inflammation is evinced by the exudation of liquor sanguinis, and when this occurs, the process cannot be cut short, but must pass through its regular stages of adhesion, suppuration, granulation, cicatrization, etc. These statements are extremely erroneous, and, indeed, may be regarded as fairly begging the question. In the first place, it is neither an admitted doctrine, nor proven by Dr. Bennett, that inflammation may not exist, in all its essential characters, without the exudation of lymph; and even when exudation has commenced, there is every reason to believe, that judicious depletion may check the effusion, and thereby *shorten* the duration of the disease, which, in a therapeutical sense, is equivalent to cutting it short.

A little attention, however, to what occurs during the forming periods of inflammation, as observed in the web of the frog's foot, and other transparent structures, will serve to place the question in its true light. An irritant applied to the part produces the following effects: 1. Contraction of the vessels; 2. Dilatation; 3. Irregular movement of the blood; 4. Stagnation; 5. Exudation. Now the question arises: Does not *inflammation* exist until the fifth or last stage?

Without entering into any speculative considerations as to the essential nature of inflammation, it may be safely affirmed that the demonstrable part of the process is *intra-vascular*, and that the exudation into the interstitial tissues is altogether secondary, is variable in character and extent, and may or may not occur. The interstitial changes embrace

exudation of lymph, adhesions, formation of vessels, suppuration, and so on; but these conditions are variable in degree and character, and are merely *results* of the primary lesion. Hence, the exudation of lymph is no more an essential part of the inflammatory process, than is the secondary changes of adhesion, suppuration, or gangrene. In illustration of this subject, it may be further remarked, that inflammation is known to occur in certain tissues without the effusion of lymph, as, for example, in cartilages (Goodsir, Redfern), and in the cornea (Virchow). And in addition to this we may add, that inflammation of the mucous and dermoid tissues may often exist with little if any exudation of normal liquor sanguinis. Indeed, the elimination from the surfaces of mucous tissues, in moderate degrees of inflammation, seem merely modified mucus, or, at most, the corpuscular variety of lymph, and therefore cannot with propriety be called *normal liquor sanguinis*. The *essentials*, therefore, of inflammation, in a practical sense, consist in the local excitement, engorgement of the vessels, stasis of blood, and a *tendency* to exudation of normal liquor sanguinis, which latter may or may not occur.

So far, therefore, as pathological conditions serve as therapeutical indications, there is no room to doubt that the essential process of inflammation is, under certain circumstances, amenable to timely and judicious medication, and that it need not, as Dr. Bennett supposes, necessarily pass through all its stages, or run a natural course. And it is quite immaterial whether we adopt or reject the proposed definition of inflammation, for it may be in technical terms correct, and yet the deductions entirely erroneous.

But the ingenious theories based on obscure definitions, and the deductions from hypothetical premises, are most certainly corrected by practical observations and clinical experience; and hence, that which the speculative pathologist attempts to establish by abstract reasoning and forced analogies, the practitioner refutes by daily experience in the treatment of disease. Thus the theorist asserts that inflammation cannot be cut short by treatment, and he proves the assertion by hypothetical conclusions; while the enlightened physician is taught by practical experience that timely depletion will either mitigate the violence, shorten the duration, or arrest the progress of inflammatory action.

Inflammation of the air-passages and pulmonary substance affords some of the most conclusive examples in support of the opinion that phlogistic disease can be arrested short of its full and complete course. Thus, a child is attacked with fever, cough, hoarseness, and difficult inspiration; this is the forming stage of croup, *prior to the effusion of lymph*, but still a state of inflammation. If such patients are bled, vomited, and purged, the symptoms subside and recovery ensues. On the contrary, if left to nature (or, according to the new theory, *supported*), the violence of morbid action rapidly increases, a false membrane is formed, and the child dies, not exhausted, but asphyxiated. These are the probabilities; and so constant

and uniform are the results, that clinical experience has established a scientific rule in practice. And the same events occur in pneumonia, both in children and adults. A world-wide experience demonstrates that this affection becomes fatal in an exact ratio with its duration prior to treatment. Grisolle observed, with the greatest accuracy, a direct relationship between the mortality in pneumonia and the period when treatment was commenced. Thus, in cases admitted within the first three days, the mortality was one in thirteen; within four days, one in eight; within five days, one in six; within six days, one in four; within seven days, one in three; and when eight days had elapsed, one-half perished! Such facts are incontrovertible. They clearly exhibit the influence of treatment (antiphlogistic) over the course of disease, in moderating its violence, and leading to a favourable result. But we need not resort to statistical evidence to establish the influence of treatment either in arresting the progress or mitigating the violence of inflammatory disease; for no amount of speculative pathology or ingenious hypotheses can subvert the clinical experience of the great body of our profession.

In attempting to form correct opinions in relation to the influence of depletion over inflammatory action, it is a very partial and erroneous view to limit our observations to what is taking place in the capillary vessels. Dr. Bennett may be able to see nothing in the inflammatory process except the effusion of lymph, nor to comprehend any therapeutical act save that of cell-transformation as observed in adhesions, suppuration, and gangrene. But the practical pathologist and therapist sees in addition the commotion produced in the general system; he does not, with Dr. Bennett, ignore the *inflammatory diathesis*; but he fully appreciates the abnormal excitement of the heart, arteries and nerves, and the relation which this condition sustains to the local disease. He is at the same time fully aware of the changes which are taking place in the capillaries; he knows that the *stasis* will be in proportion to the violence with which blood is not only *drawn* but *forced* into the diseased part, and that the exudation will in turn bear a direct relationship to the degree and extent of stagnation. Furthermore, it is evident that the destructive tendencies of inflammation, either in arresting the functional action of important viscera, or in those secondary changes connected with adhesions, suppuration, gangrene, and so on, will be proportioned to the extent of effusion, not less than the recuperative powers of the system. Hence the philosophical practitioner seeks to limit the morbid process by lessening the inflammatory diathesis, where the disease is not purely local, and for this purpose he reduces vascular and nervous excitement generally, and either arrests inflammation or limits its violence and destructive tendencies.

It is a most palpable and serious error to attempt to draw indications of treatment from what occurs during the stage of cell-transformation, a period when the essential process of inflammation has ceased, and when

brandy and beef-tea might be tolerated or even demanded. It is thus by looking alone to the terminal acts of inflammation—the conservative or destructive part of the process, as the case may be—that Dr. Bennett and his coadjutors have committed the great error of rejecting depletory treatment, and encouraging an indiscriminate stimulation. Whereas, the discriminating practitioner adapts his remedial agents to the constitution of the patient, and the stage and character of the disease; and thus he is often obliged to deplete in one stage, and stimulate in another, thereby protecting the system from the violence of over-action, and sustaining the vital powers during resolution.

The following is Dr. Bennett's fifth and last proposition:—

5. "*That all positive knowledge of the experience of the past, as well as the more recent observations of the present day, alike establish the truth of the preceding principles as guides for the future.*"

Dr. Bennett here reaches the culminating point. Having, as he assumes, established his principles, he deduces the preceding proposition, and remarks that "the more exact observations of the present day" establish these doctrines as guides in practice. What Dr. Bennett means by "the more exact observations of the present day" is, I presume, his own opinions; at least the propositions which constitute the basis of his views are purely his own for, with the exception of Dr. Todd, and some of the experimentalists of the school of Vienna, they have certainly received no countenance from the lights of our profession. We do not find Alison, Christison, Watson, Copland, Jenner, Walshe, Barlow, etc., advocating such principles; nor have they thus far received the sanction of that test which all rules of practice must undergo—the *experience of the great mass of practitioners*. Closet-practitioners, and men striving for notoriety, may weave a thousand intangible but spacious theories, but it requires the crucible of clinical experience to separate the dross from the gold.

We might fairly leave this fifth proposition to its own inevitable fate, for having, I think, shown the fallacy of the hypotheses on which it rests, the conclusions must necessarily fall with the premises; but inasmuch as Dr. Bennett has reserved for this concluding part certain *statistical* evidence, embracing his own experience, it is proper to give it that attention which the importance of the subject so clearly merits.

Most observers perceive and acknowledge the difficulties and uncertainties attending *statistics* connected with the practice of medicine; and we shall presently see to what extent these difficulties authorize us, in the main, to reject such evidence as faulty and unreliable. And however much we may admire the patience and industry of a Louis in attempting to create the *numerical system*, it must still be admitted that such facts are of necessity partial and inconclusive, and therefore unreliable data in the practice of medicine. In the language quoted by the distinguished Dr. Watson, it is better to *watch* than to *count*.

But as the opponents of depletory remedies in the treatment of inflammation depend largely on statistical facts to support their views, it becomes necessary to advert to this branch of the subject. The particular disease which has been seized upon is *pneumonia*; and it will be found, upon examination of the subject, that it has proved a very prolific, if not a very conclusive theme, and that the facts elicited are as contradictory as they are numerous. It is unnecessary to bring up in review all that has been published on this subject; but it will be sufficient to notice the most striking statements, and especially such as have been chiefly relied on by those opposed to bloodletting. In the *British and Foreign Medico-Chirurgical Review*, July, 1858, is a copious summary of this kind of evidence, from which I shall draw some materials bearing on this question; but it is the statements of contemporaneous observers, such as Bouillaud, Dietl, Wunderlich, Bennett, Bell, and Balfour, which are most valuable.

Bouillaud, of the French school, may fairly be considered, as *par excellence*, the leader of the bloodletting party of the present day; and his bleeding, *coup sur coup*, may be deemed an extreme test of the influence which severe depletion will exercise over pneumonia.

He thus states his method in pneumonia:—

“Bleed in the morning of the *first day* to sixteen, and in the evening to twelve or sixteen ounces. In the interval cup to the same amount, or apply thirty leeches. On the *second day* bleed again, and if pain still continues cup or leech. The disease, fortunately, for the most part yields on the *third day*. If otherwise, don't hesitate, but bleed again; but usually it is better to apply a large blister. As a rule you must not give up bleeding until fever, pain, and dyspnœa have almost ceased.”¹

Bouillaud states that he usually abstracted from four to five pounds of blood, the largest quantity being ten pounds, which was taken from a patient who recovered. He records the mortality to have been in 102 cases 1 in $8\frac{1}{2}$.

This must be considered heroic bloodletting; and when we remember that it was indiscriminately employed, having but little reference to age, sex, stage of disease, or constitution of the patient, it may be considered as remarkable success, and exhibits a point of great significance in the treatment of the disease. If, indeed, bloodletting is so unconditionally evil, as Dr. Bennett would have us believe, Bouillaud's patients should have all died; but, instead of this disastrous result, we find a mortality of only 1 in $8\frac{1}{2}$, which is fair success for hospital practice.

Let us now look at another extreme. Dr. Balfour observed the results of treatment in the Homœopathic Hospital of Vienna, under the care of Dr. Fleischmann, and states the mortality at 1 in $6\frac{2}{3}$. It will, of course, be conceded that homœopathic treatment is equivalent to non-interference,

¹ *Medico-Chirurgical Review*, from *Traité de Nosographie*.

and we thus acquire some knowledge of the natural history of the disease. In this example, it appears, the mortality was large, which teaches two facts—that homœopathic treatment is unsuccessful, and that the disease in that instance tended largely to a fatal result.

Now, by contrasting Bouillaud's bloodletting with Fleischmann's negative treatment, it will be perceived that the results are favourable to the former; and if any induction is warrantable from such facts, it is that even excessive and indiscriminate bloodletting is better than unaided nature, and, therefore, depletion possesses positive curative powers.

The statistical evidence on which Dr. Bennett places most reliance (except his own) is that furnished by Dr. Dietl, of Vienna, which is classed under the head of "*treatment by diet.*" It appears that Dr. Dietl adopted three methods of experimental treatment: 1. By venesection. 2. By tartar emetic. 3. Dieting, with mild ptisans. The following is a general summary. Whole number treated, 380:—

1. By venesection 85, of whom 17 died = 1 in 5.
2. By tartar emetic 106, of whom 22 died = 1 in 5.22.
3. By diet 175, of whom 14 died = 1 in 13½.

According to the face of this statement *nature* cures between two and three times as many cases of pneumonia as bleeding and antimony; *ergo*, bleeding and antimony should be abandoned, and the disease left to the recuperative powers of nature. There are, however, certain considerations which materially impair the force of Dietl's conclusions, and take from his figures their seeming authority. It will be remarked that Dr. Dietl treated his cases exclusively by a single remedy—bleeding or antimony—and these were evidently employed indiscriminately, and consequently must have been prejudicial in a certain proportion of the patients. Nor is it less important to remark, that no judicious practitioner would limit himself to a single agent, to be employed in all stages and conditions; on the contrary, he would resort to a combination of remedies to meet the indications of different cases and stages of disease. It is not surprising, therefore, that the indiscriminate use (or rather *abuse*) of a single *active* agent should give rise to a mortality equal to that reported by Dietl; nor is it at all strange that unaided nature should, in the aggregate, produce more favourable results than the palpable abuse of the most active agents we possess. Hence, Dr. Dietl's figures cease to be authoritative, and his conclusions must be regarded as fallacious.

In addition to these considerations, Dietl's conclusions are entirely neutralized by the reports of Wunderlich, of Liepsic. His cases may be thus stated:—

1. In 114 cases loss of blood occurred (by general and local bleeding, epistaxis, menstruation), of whom 9 died = 1 in 12.6.
2. 47 cases treated by venesection, 3 died = 1 in 15.6.
3. 76 cases without loss of blood, 13 died = 1 in 5.8.

It is also remarked by Professor Wunderlich, that bloodletting exercised a marked influence over the duration of the fever, or in promoting *defervescence*.

Without attaching any very great intrinsic importance to these statements, they are evidently entitled to as much credit as those of Dietl; and as they are precisely the reverse of the results obtained by the latter, they at least serve to place statistical inquiries in their true light, and to render us cautious in attaching too much confidence to *figures*.

But let us turn to the results of Dr. Bennett's practice. He informs us that he now abandons all effort to cut the disease short, but during excitement gives small doses of salines, *with the view of diminishing the viscosity of the blood*; and, as soon as the pulse becomes soft, orders beef-tea and wine. This course, the author states, he has carried on in the clinical wards of the Royal Infirmary during the last eight years, in which time he has treated 65 cases, of which 3 died, giving a mortality of 1 in $21\frac{2}{3}$. The average age was 31; average duration of single uncomplicated cases $14\frac{1}{2}$ days, and of the double uncomplicated, 21 days.

From these statistical statements, Dr. Bennett draws the following conclusions:—

1. That the result of a vigorous antiphlogistic treatment, as formerly practised, is a mortality of 1 in 3.
2. That the result of treatment by large doses of tartar emetic, according to Rasori and Dietl, is 1 in 5; but according to Laennec, 1 in 10.
3. That the result of moderate bleeding, as in the treatment of Grisolle, is a mortality of 1 in $6\frac{1}{2}$.
4. The result of dietetic treatment, with occasional bleedings and emetics, by Skoda, is 1 in 7.
5. That Dietl's dietetic treatment gives a mortality of 1 in 13.
6. That Dr. Bennett's treatment gives a mortality of 1 in $21\frac{2}{3}$.

It is evident from these statements that Dr. Bennett believes he has attained the highest degree of success in the treatment of pneumonia, and that Dietl stands next, the mortality being, respectively, 1 in $21\frac{2}{3}$ and 1 in 13. We have the facts before us, however, to prove that other persons, and by depletory treatment, too, have secured still more favourable results, and, so far as statistics are reliable, establish the correctness of antiphlogistic treatment.

By referring to the *British and Foreign Medico-Chirurgical Review*, July, 1858, the following facts will be found:—

Reuf treated 94 cases of pneumonia by bleeding and antimony, of whom 5 died = 1 in $18\frac{4}{5}$.

Bang, of Copenhagen, treated 54 cases with antimony and bleeding, of whom 2 died = 1 in 27.

Trousseau treated 52 cases in Hôtel Dieu, with bleeding and antimony, 2 died, = 1 in 26.

Wossildo treated 76 cases, between the ages of seventeen and seventy (5 above sixty), by general and local bleeding and antimony, *none died!*

Burkart treated 60 cases by bloodletting, in 1854, when the type was inflammatory, with only one death, and that one was complicated with tubercles; hence he cured 59 uncomplicated cases without the loss of one.

We may now fairly ask, What is the basis of Dr. Bennett's conclusion, that his success has been greater than others? In vain may he appeal to former statistics of the Royal Infirmary, showing a mortality of 1 in 3, or to the results of treatment by Chomel, Louis, and Grisolle, where powerful remedies were almost indiscriminately employed; for, notwithstanding these statements, we find better results in the hands of other practitioners, and which equally deserve to be contrasted with the statistics of Dietl and Bennett. Thus, Dr. Bennett loses 1 in $21\frac{2}{3}$, while Trousseau exhibits a mortality of only 1 in 26; Bang, 1 in 27; Burkart, 1 in 59; Wossildo, 1 in 76! Surely, Dr. Bennett's success, compared with these triumphant results, dwindles to insignificance; and if we are to be guided by statistical evidence, then should we bleed and give antimony, to secure the most favourable rates of mortality.

The result of Dr. Bennett's treatment requires a special remark. In the first place, his cases were very few in number, being only 65 in eight years, which is evidence that pneumonia did not prevail as an epidemic, and consequently that he encountered only sporadic or mild cases. In addition to this, the patients were comparatively young, the average age being thirty-one; and 55 were uncomplicated, of which 40 were single. It is stated by Dr. Bennett, also, that six of the cases which recovered were bled and subjected to antiphlogistic treatment before entering the Infirmary—a fact of no small importance in estimating the general results. There is another significant fact which deserves to be noted in this connection. It is distinctly stated in the Review already referred to, that during the period covered by Dr. Bennett's cases, pneumonia was remarkably mild throughout Scotland, and that the mixed treatment (bleeding, antimony, and mercury) adopted by Dr. Bell, of Glasgow, was equally successful with the non-bleeding treatment by Bennett.

It will not, perhaps, be uninteresting to remark, that historical reminiscences often throw great light on the progress of medical doctrines; and in connection with our present subject, a very brief retrospect will show that Dr. Bennett's antecedents have been very different from his present position. It is true, there is no offence against either good morals or sound philosophy in changing opinions as our views become enlarged and matured; but a change so complete and radical as exhibited in the present example, and based too, in both instances, on practical observations, is too remarkable to pass unnoticed. In fact, personal history becomes in some sense a part of science, especially when great innovations are based on individual experience. In his *Clinical Medicine*, published in February,

1858, Dr. Bennett declares that the course of treatment now recommended has been pursued by him for the past *eight years*. We find, however, in the *Edinburgh Monthly Journal*, August, 1851 (six and a half years before), the following language employed by Dr. Bennett in a clinical lecture:—

“I have on a former occasion pointed out the rule which, as it appears to me, should guide you with regard to bleeding in pneumonia. If you are called to a case at a very early period before exudation is poured out, and before dulness as its physical sign is characterized, but when, notwithstanding, there have been rigors, embarrassment of respiration, more or less pain in the side, and commencing crepitation, *then bleeding will often cut the disease short.*” (I have Italicized the most important part.)

How different Dr. Bennett in 1851, and Dr. Bennett in 1858! *Then* his clinical experience taught the important practical rule that bleeding, at the proper time, *would* cut short a pneumonia; *now* he emphatically declares that no such result can possibly ensue from depletory practice! In confirmation of his opinion held in 1851, he details one decided case in which the abstraction of 3xv of blood was followed by marked relief, *even crepitation subsided*, and the patient rapidly recovered. The question, therefore, naturally arises, did Dr. Bennett observe, in 1851, that pneumonias were cut short by bleeding, or was he incompetent, as a diagnostician, to decide what actually occurred? Certainly we cannot doubt either his veracity, or his skill to trace the different stages of pneumonia, by the physical signs and general symptoms; and his observations at that period, unobscured as they were by a favourite theory, or deceptive statistics, will remain an incontrovertible disproof of his present speculative opinions. Nor is Dr. Bennett consistent with himself in his new position. He treats pneumonia, during the state of excitement, by salines, to diminish the viscosity of the blood; but, according to his theory, he should at the same time give brandy and beef-tea, in order to secure a large flow of blood. Truly, theory and practice do *not* always agree.

Some attempt has been made in our own country to develop the statistics of pneumonia. Dr. Austin Flint, Professor of Clinical Medicine in the New Orleans Medical College, has published a series of fifteen cases, occurring in the Charity Hospital, New Orleans, which were treated from the beginning with quinine, opium, stimulants and nutrients. It will be seen, however, by reference to Dr. Flint's commentaries on the cases, that he does not claim them as a fair type of the disease in general, nor does he assume to exclude, in appropriate cases, depletory practice. On the contrary, the cases reported by Dr. Flint were, as he admits, evidently broken down in constitution, having long suffered with intermittent fever, and from the influences of exposure and a hot climate. We can readily understand, therefore, that these and similar examples, where miasmatic diseases prevail, may often require tonics and stimulants from the very onset, and entirely forbid all depletion.

Such facts, however, are far from establishing a general rule; for it is well known that pneumonia occurring under different climatic and endemic influences requires a very different course. This general fact is well illustrated in what occurred in the Louisville Marine Hospital, in 1855. The report for that year shows the number of cases to have been 37, of whom 18 died. I am not in possession of all the facts connected with these cases, but it has been stated that Dr. Flint, who had charge of the hospital during the winter, treated them mainly by the exhibition of opiates, and without bleeding, antimony, or mercury. It was, therefore, essentially an expectant and anodyne course; and the result contrasts strongly with the treatment in the New Orleans cases. I am not aware of any example on record in which the modifications of disease, demanding a corresponding difference of treatment, is more clearly exhibited than in these two classes of cases.

It becomes an important question for the practitioner to decide, how far confidence can be placed in medical statistics, especially such as bear on this subject, and to what extent such evidence can be made a guide in the treatment of disease? An examination of the statistics of pneumonia, which occupy so important a position in Dr. Bennett's theory, will reveal results so variable and contradictory as to deprive them of the slightest claim to authority. Thus, without depletion, Dr. Bennett's statistics show a mortality of 1 in $21\frac{1}{4}$; Dietl's, 1 in 13; the homœopathic 1 in 6; and the non-bleeding plan in Vienna, in 1856, 1 in 4. With antimony, bleeding, &c., Grisolle lost 1 in 8, Dr. Bell 1 in 17.7, Trousseau 1 in 26, Burkart 1 in 60, Wossildo none in 76. Pneumonia treated by inhalation of chloroform furnishes the following mortality: in the hands of Baumgärtner 1 in 10, Varrentrapp 1 in 23, Wucherer 1 in 90! In the Royal Infirmary, Edinburgh, former statistics show a mortality of 1 in 3; and this constitutes, mainly, the foundation for Dr. Bennett's denunciation of depletory treatment.

In addition to this, Kissel treated 112 cases, with a mortality of $5=1$ in $22\frac{2}{3}$. When the urine was alkaline he gave iron; when it was acid he gave copper.

Here is exhibited a very wide range of figures. The non-bleeding plan varies from 1 in 4 to 1 in $21\frac{2}{3}$; the antiphlogistic from 1 in 3 to 1 in 90. Are not these results too variable to constitute any sound basis of practice? If we take Dr. Bennett's statistics, we would certainly not deplete; if we take Wossildo's results as the guide, we will as certainly resort to blood-letting; but if we chance to adopt the tables of Wucherer, then we will administer chloroform! or iron and copper, if we depend on Kissel. Each partisan will find his theory fully sustained by these figures; but the judicious practitioner will perceive that some unseen agency has modified the results, and that the mere figures are but so many fallacies. It is evident, therefore, that the statistics of pneumonia, as a whole, are utterly worthless and unreliable as practical guides.

If we seek an explanation of these contradictory results in the treatment of pneumonia, it will be found in the numerous qualifying conditions connected with age, season, climate, epidemic and endemic influences, early treatment, stage, extent, and complications of the disease. And to these conditions we must add, in a general sense, the *individuality* of each case; indeed, so great are the differences in constitutions, that no two examples will exhibit the same characteristics throughout, nor will they admit of precisely the same method of treatment. And it is a due appreciation of these more minute shades of differences, as well as the broad distinctions observed in the varying *forms* of the disease, that constitutes the truly skillful physician, and which enables him to meet the emergencies of each case, instead of relying on conclusions drawn from *groups* of cases.

Viewing nationalities in a somewhat prejudiced light, a critical writer intimates that the English think more of some other case than the one under treatment, while the French think more of the disease than of the patient; hence the former individualize the disease, the latter generalize the patient; but the true course is that indicated by Hufeland, to *generalize the disease and individualize the patient*. It is quite immaterial to our present purposes, whether these distinctions exist among French, Germans, and English, or not; but we cannot fail to observe their strong development in individual writers. Statisticians rob each case of its individuality, and cast it upon the sea of uncertainties pertaining to others of a different character. Thus one series will all be bled, another will receive tartar emetic, and a third left to the chances of nature. In the first class, some are bled who should have been stimulated; in the second, tartar emetic is administered when bleeding would have been preferable; and in the third class, some are permitted to die from mere over-action. In this blundering, if not criminal procedure, individuality is ignored, and the practitioner prescribes for a mere *name*, leaving the patient to the mercies of chance or fate.

It is evident, therefore, that a rational treatment must secure to each case its own individuality; and as the shades of differences, and the corresponding modifications of treatment cannot be expressed in *groups*, statistics, in this sense, become simply an impossibility. For example, bleeding, antimony, mercury, and blisters, may be demanded in one case; quinine, opium, and wine in the next; a third may require but little interference, except a well-regulated diet with moderate stimulants; and so on, *ad infinitum*. The treatment of pneumonia demands not a single but many agents; and he who would attempt to develop results by statistics, will be required to make each group a *unit*. It is the proper *combination* of remedies, and not a single agent or mode of practice, which is capable of securing the best results in the treatment of disease.

The preceding considerations render it abundantly evident, that mere statistical tables have not furnished the class of facts on which practitioners

can rely in the selection of remedies; and, indeed, the only important revelation which has been made is, that, under certain conditions pneumonia manifests a stronger tendency to spontaneous recovery than could otherwise have been known. Dietl, for example, under a system of mere diet, reports the mortality of only 1 in 13, which is more favourable than resulted from the systematic course pursued in Paris. But the presumption is very strong that Dietl's cases were of that grade which tend to spontaneous recovery; at least we have the important contrary fact, that in 1856, the non-bleeding treatment of pneumonia in Vienna was very unsuccessful, the mortality being 1 in 4! Doubtless, however, uncomplicated pneumonia, when single and occurring in good constitutions, would generally recover without the aid of medicines; *and the same is true of a large proportion of other diseases.* Many cases of typhoid fever, measles, scarlatina, smallpox, and so on, would recover without the intervention of art, or the aid of a physician. But these facts establish nothing in the premises; nor can any evidence be adduced to prove that suffering may not be mitigated, and life often saved by the timely and judicious application of remedial agents. Some examples of disease may safely be intrusted to the recuperative powers of nature; but even in this sense there are few cases which might not be benefited by proper medication. Some cases require strong remedies, others weak ones; and it is the function of the enlightened physician to determine when his strong and when his weaker agents are to be brought into requisition; when he is to trust mainly to the *vis medicatrix naturæ*; when to sustain the faltering powers of life; or when to subdue vehement action, which, by its own violence, threatens destruction to the animal economy.

It has been observed, however, that the treatment of inflammatory diseases has materially changed within recent periods, depletion having fallen into comparative disuse, and that this important modification is due to a better comprehension of morbid action. This observation, although only partially true, might be admitted without solving the question at issue. Medical science, and, based on this, medical *art*, is progressive; we, in modern times, know more of pathological changes and therapeutical actions than did our predecessors; and hence we measure the influence of the one by the character and degree of the other with more accuracy than formerly. The natural history of disease has been more carefully studied; and its tendency to spontaneous cure, or a fatal termination, materially modify therapeutical applications. Hence, a juster comprehension of pathological changes, and a broader and more enlightened experience, have taught us that essential fever is not to be cured by depletion; that remittent fever is, as a rule, more efficiently treated by quinine than mercury and bleeding; and so, too, have we learned that excessive depletion in pneumonia may often be dispensed with, and a more conservative course advantageously adopted.

But the question recurs, whether the diminished employment of blood-letting has resulted exclusively from the advanced state of pathology, or whether it is not due to a change in the *type* of disease, whereby depletion is less frequently demanded. Drs. Alison, Christison, Watson, and others, declare that the type of inflammatory affections has changed, having become less *sthenic*, and, consequently, the lowered grade of action no longer requires copious depletion; while on the contrary, all this is denied by Dr. Bennett, who asserts that no change of type has occurred, but that the modification of practice is due to a more enlightened pathology; and on that advanced pathology he predicates his peculiar views.

These are questions not easily settled by the mere pathologist or the logician. Dr. Bennett may demonstrate that cell-action, and the laws governing the process of exudation, are the same now that they were in the days of Cullen and Gregory, or even of Hippocrates and Æsculapius; but the observation is radically defective, because it ceases precisely where it should have begun. The demonstration should ascend from the local affection to the constitutional reaction; and thus by measuring the degree of excitement, reveal the diathesis of the disease. In fact, the evidence that the type of disease fluctuates in intensity, must be derived from personal observation and experience, in relation to the conditions of the general system, rather than the revelations of the scalpel or the microscope, concerning the minute changes in inflammation. And it is no more possible to establish such a fact by *statistical* evidence, than it would be in the same manner to prove the exact agency of psychological influences in the production or aggravation of disease. But there is an experience and observation altogether above mere arithmetical calculations; and it is from this broad and reliable evidence that we learn the fluctuations of disease in different seasons, years, and periods of time.

The variation of inflammatory affections may be clearly observed, on a limited scale, in what occurs during the different seasons of a single year. Thus it is well known that inflammatory diseases bear and require more antiphlogistic treatment during winter than summer. But still more distinctly are these variations observable in different years; indeed, every practitioner must have remarked that the same classes of disease manifest a much higher grade of action, and require more depletion during some years than others. And, if this is true of seasons and years, there is no obvious reason why the same influences may not extend through longer periods or cycles of time. In our own country we have numerous illustrations bearing on this question of the change of the type and character of disease. Thus it is well known that, since the prevalence of Asiatic cholera in 1832, there has been manifested a greater degree of irritability of the alimentary canal, and consequently diminished tolerance of cathartic medicine. Purgatives have fallen into disuse, since the days of Hamilton, even to a greater extent than has bloodletting since the days of Cullen and Gregory. And

it may be safely affirmed that the change of practice in this respect cannot be ascribed to an improved pathology, but to a broad and enlightened experience growing out of an obvious change in the *type* of disease.

In the western and southern portions of the United States another and even more striking change has occurred. The endemic fevers of this vast region were originally of the periodical type; but as early as 1842 we were invaded by well-defined continued (typhoid) fever, which in many localities superseded the periodical fevers. The continued type predominated in many localities for a period of ten years, since which time it has gradually diminished, while periodical fever again becomes more common. I do not assert that these changes were radical and complete in every district, but the predominance of the two types occurred, as I have stated, in many regions of country, and the typhoid element seems to have permanently impressed most of the diseases incident to the climate. And this important modification of disease demanded at once a radical change of treatment. The preparations of bark and mercury, together with bloodletting, were no longer efficient; but instead of these, the employment of stimulants and nutrients became the leading agents. Quinine, so efficacious in periodical fevers, was not only inefficient in the new form of disease, but was often found positively pernicious; mercury was seldom required, and frequently wholly inadmissible. This great change of treatment was not due to an improved pathology, but it arose from the introduction of a new form of disease, and experience soon indicated the necessary changes of treatment.

But still another modification in the type of disease has occurred here and elsewhere. Practitioners have observed, for some years past, a *nervous* type, with often a decided tendency to prostration, so much so, indeed, that depletion must be resorted to cautiously, or entirely interdicted.

Now it is evident, from the concurrent statements of writers in Europe and America, that these changes are general and common to both countries; and that a corresponding modification of treatment has occurred, co-extensive with the changes in the type of disease. Typhoid fever has of late years spread over England, and has been fully recognized by the practitioners generally. In 1845, the writer of these remarks observed cases of typhoid fever in the London Fever Hospital; but they were limited in number, and, perhaps, not well defined, for Dr. Tweedie remarked that the disease had not been recognized, and that they made no distinction between the typhus and typhoid forms. Soon, however, typhoid fever multiplied, and its existence was fully recognized by Dr. Jenner and other observers.

I may also mention here, as evidence of the change which the *type* of fever undergoes, that Dr. Tweedie's report for the year 1845, shows the low form of disease by the large quantity of stimulants demanded. He states that, in the epidemic of 1843, when 1,100 patients were admitted, the quantity of wine administered was about 1,800 ounces, and 60 of

brandy; while the next year, although not half the number were admitted, they consumed 14,000 ounces of wine, and 760 of brandy, besides gin and porter! No fact could be more striking and conclusive than this.

It ceases, therefore, to be a matter of surprise or doubt that these zymotic causes, with others probably unknown, should modify the *type* of disease and require treatment greatly changed in character. The choleraic and typhous poisons, to say nothing of the causes which have so extensively modified the nervous system, must be regarded as fully competent to effect these important changes.

It is contended, however, by Dr. Bennett, that *inflammation* is always essentially the same, and hence there has been no change in the *type* of disease. It is very true, indeed, that the elementary actions characteristic of inflammation must necessarily remain unchanged; that is, the adhesion of the corpuscles, distension of vessels, stagnation of blood, and, finally, the exudation of lymph, are the same in the days of Bennett that they were in those of Cullen. But this does not embrace the main question, for it is not the local changes occurring in an inflamed tissue which are supposed to have undergone changes, but it is the condition of the *general system*—of innervation, circulation, and all the vital functions. These become, from general causes, depressed; and although there may be no change in the microscopic appearances of inflammation, nevertheless the *reaction* is less intense, the tendency is to depression, and depletion is less demanded. Hence the *type* of disease may change, while the minute process of inflammation remains unaltered.

It is not contended, however, that this change of type will be observed in every example of disease; on the contrary, we still witness the old division of sthenic and asthenic inflammations. The former, however, have diminished, until, finally, the latter predominate; or what would, perhaps, be more correct, there is a general lowering of the grade of action, which requires less depletion than did the same classes of disease in former years.

At the same time, I am strongly inclined to believe that the great outcry against bleeding has driven us to the opposite extreme, and we now deplete less than the interests of our patients frequently require. With the prevailing aversion to bleeding, cases are liable to be overlooked, and depletion neglected from sheer habit. Dr. Christison clearly proves, within his own personal experience, that the synocha of Cullen has several times recurred, and each time demanding depletion. But he who would regard that form of fever as a *myth*, would not recognize its new introduction, and therefore would fail to meet its exigencies.

The question of bloodletting in inflammation has been discussed almost exclusively in relation to pneumonia; but, notwithstanding the evident good effects of depletion in certain forms of that disease, it is not the affection which most clearly illustrates the powers of depressing treatment. The reason of this is, that pneumonia is so exceedingly variable in its tendencies

as to defy the most careful observer in his attempts to tabulate results. But for the purpose of clearer illustrations, let us apply the same principles to other forms of inflammation, such, for example, as encephalitis, hepatitis, peritonitis, and gastritis. Would it be contended that, in a case of inflammation of the brain, characterized by a full and strong pulse, throbbing carotids, active delirium, etc., *stimulants* and *nutrients* should be employed, for the purpose of promoting cell-action? It would, probably, be difficult to find a practitioner, since the days of Asclepiades, not excepting Dr. Bennett himself, who would not bleed in active phrenitis, cover the abdomen with leeches in abdominal inflammation, or apply cups in hepatitis. And if this be true, the same *principles* should regulate the treatment of pneumonia, varied according to the tendencies of individual cases. In an example of active pneumonia, occurring in a robust constitution, with full reaction, hot skin, and oppressed breathing, no physician would dare withhold depletion and substitute stimulation. The instincts of our nature, to say nothing of science, would forbid it, and the opinions of enlightened physicians would declare it *malpractice*.

The treatment of pneumonia, I need scarcely add, must vary with its forms and modifications. Let us assume the existence of the following varieties:—

1. Sthenic pneumonia.
2. Asthenic pneumonia.
3. Latent pneumonia.
4. Specific pneumonia (typhoid, miasmatic, etc.).
5. Diathetic pneumonia (rheumatic, scrofulous, etc.).

In these five species will be found therapeutical indications widely different; and the discriminating practitioner will perceive the necessity of bleeding in one, of giving quinine and opium in another, while still another class will demand specific treatment. It is freely admitted that in the milder forms of pneumonia but little treatment is demanded, and certainly bloodletting may often be omitted; but in the graver varieties the agents must be more active, or the patients will be destroyed by the inherent force of the disease. In the milder forms of all diseases, the *vis naturæ* may be sufficient to overcome morbid action; in others, again, the same *vis naturæ* must be protected from the destructive tendencies of *over-action*; while in another class, characterized by debility, the powers of nature must be sustained by stimulating agents. These are the guides which physicians, unbiased by speculative doctrines, always recognize, and which constitute the basis of enlightened practice.

The denunciation of bloodletting is nothing new. From the days of Chrysippus and Erasistratus to those of Bennett and Dietl, parties have often risen and flourished on a species of monomania on the subject of bleeding. In Rome, in the days of Galen, the opposition to depletion was

fierce and bitter, and the *blood-funkers*,¹ as they were humorously called, so far predominated as to prevent depletion in the most urgent cases. And, finally, the predominance of Arabian over Greek medicine caused blood-letting, in any efficient form, to be abandoned. But, in the beginning of the sixteenth century, Pierre Brissot, a physician of Paris, finding most of his pleuritic patients die, ventured to revive the Greek practice of bleeding. This created, says Renouard, a great uproar in the medical world, and violent controversies ensued. And more recently, the Brunonian doctrine, although it does entirely exclude bleeding, is based measurably on the employment of stimulants to remove *debility*. In our own country the outcry of the blood-funkers has been as excruciating as the *antimonial martyr-ology* of Guy Patin. Originating in the profoundest depths of ignorance and presumption, it has gradually risen to a more imposing but not less false position; and while struggling to sustain a system based on the relics of the dark ages of medicine, such as Galen and Brissot had to combat, the Edinburgh defection comes opportunely to their support. Dr. Bennett may have the satisfaction of knowing that his doctrines, in the United States, are eagerly embraced by the empirics, while the regular profession repudiate them as false and mischievous. The American *blood-funkers* find great consolation in the doctrines of the Edinburgh and German medical *Illuminati*.

But let us hope the end is not yet. Theory and practice do not always accord. The celebrated blood-funker has been attacked in a tender way. Inflammation came stealthily upon him, when lo! Dr. Bennett's case *demand*s bloodletting! His colleague, Professor Miller, informs us that his sthenic constitution nobly sustained depletion. Thus, the hand of Providence becomes a more potent teacher than statistical tables or microscopic revelations.

¹ Chambers, Med.-Chir. Rev., Oct. 1858.

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Samuel

Henry
Henry M. B.

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